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an attempt which, accepting the complete right and autonomy of knowing and of logic in its own field, tries to see what this field of knowledge and reflective intelligence is and means as a specific type of behavior in a more inclusive scheme of behavior.

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ON SECULAR COOLING AS AN ILLUSTRATION OF THE METHODS OF APPLIED MATHEMATICS

FRIENDS and critics to whom I have shown my article on "The Methods of Applied Mathematics"¹ have usually made remarks to the effect that the most interesting part of the essay would be its application. It is one thing for the philosopher to abstract and classify the methods of the various specialist sciences, and quite another for him to show that this process is of definite practical use, by giving examples of the manner in which the scientific specialist makes grave and serious errors through ignoring the rightful methods of his science. This essay is an attempt to accomplish the latter purpose.

Amongst the examples of the misapplication of the methods of applied mathematics, none is more interesting or more cogent than that concerning the secular cooling of the earth. Owing to recent discoveries in radioactivity, the discussion has become, in some respects, of purely academic interest. But many of the old fallacies are still extant, and, to act as a check on future errors of the same kind, it will serve a useful purpose to examine the fallacy from the foundation and to show in what manner it arises from the misapplication of mathematical method.

The basis of this well-known method of Lord Kelvin² may be briefly described as an attempt, from observations of the increase in temperature as we penetrate into the crust of the earth, to calculate the time that had elapsed since the consistentior status, or, in other words, since a permanent solid exterior first formed on the surface of the earth.

The present essay is not concerned with the calculation, but with the premises on which it is based. It is intended to show, not only that this speculation has been proved invalid by recent discoveries, but that, if the necessary limitation of mathematical methods had been clearly understood, it would never have been put forward.

¹ This JOURNAL, September 30, 1909.

² See *Trans. Royal Soc. Edinburgh*, Vol. 23, republished in "Mathematical and Physical Papers," Vol. 3, to which paging references are made.

The basis of the calculation is an equation of Fourier, which was designed as a solution of the hypothetical problem of "the rate of variation of temperature at any point, in a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two constant values on two sides of an infinite plane."³

I do not think that the essential conditions for the earth, in the vast unknown time since the consistentior status, to approximate to this ideal, have ever been clearly and explicitly stated. Some of them will appear at first sight to be truisms and some have been overlooked as a matter of habit, because, in the vast majority of the operations of mathematical physics, they are of the obvious kind that can be tacitly assumed.

The following statement of these implications makes no pretense to be exhaustive; but it will indicate that, in such a case as the present problem, much is liable to be assumed without sufficient and adequate examination. In order that Fourier's equation should be applicable to the concrete case of the real earth during geologic time, this real earth must satisfy, amongst others, the following conditions: (1) The spherical rotating body must for practical purposes correspond with this infinite plane. (2) This same earth must have constant conductivity and specific heat both in time and place. (3) The earth must be mathematically solid. There must be an absence of convective action both in time and place. (4) The temperature of the surface and of the heated interior at a given distance from the surface must be constant for both time and place. (5) The variations in temperature gradient must depend on conductivity and specific heat only, and the process of averaging the varying temperature gradients must be valid. (6) There must be no source of heat other than that originally contained in this solid of known temperature and known specific heat.

It is highly probable that, in any ordinary instance where conductivity formulæ would be used, these assumptions would be valid. In the ideal case of the cooling, within reasonable limits of time, of a large suddenly extruded mass of lava, no reasonable objection could be raised on any of these grounds. But, in the case of the earth cooling through geologic eras, we shall find that no single one of them is entirely satisfied, and that some occasional errors are so great as to invalidate the calculation. I have no competence (and if I had it would be outside the scope of this paper) to examine the necessary modifications dependent upon each of the sources of error; but ordinary application of common-sense reasoning will show

³ *Ib.*, p. 301.

that, with some of them, a small variation from mathematical exactitude will, if acting for a sufficient time, make great differences in the result. To show this we will examine our six necessary conditions separately.

1. The question of the earth as a sphere is too mathematical for treatment here. This particular difference from ideal conditions does not appear to affect the argument if the others are satisfied; but Professor Perry has shown that, if we assume a higher conductivity for the earth's interior, the spherical nature of the earth must be taken into account.⁴ This source of uncertainty, however, is small compared with those that follow.

2. The criterion of constant conductivity is unproven and it is difficult to see how proof can be obtained. On so speculative a question, no definite conclusion seems possible, and the only methods of attacking such a problem would seem to be found in general arguments based on probability. I doubt if it is possible to state all the conditions that might conceivably affect the heat conductivity in the interior of the earth. There are so many unknown factors. But we can reasonably be sure that it will be affected by at least the following three: change of chemical constitution, rise of temperature, enormous increase of pressure. Let us examine the effect of each of these factors.

It is superfluous to remark that we have no direct knowledge of the earth's interior, or that we can only speculate. But, in any speculation of this kind, we are entitled to take as our starting-point the ascertained fact of the high density of the earth (circ. 5.2) which is about double the estimated density of the crust.

This higher density may conceivably be due to one of two causes, either change in chemical constitutions, or a great change in volume due to the superincumbent pressure. Of these possibilities the former would appear to be the more probable. There are good theoretical reasons for thinking that there is a limit to the compressibility of any substance, however great the pressure may be. If this change in density be, in the main, due to change of composition, it follows that the substances in the interior of the earth must be metallic.⁵ There are no known non-metallic substances of a density greater than 5. The presence of any considerable proportion of metal would enormously increase the conductivity.

Change of pressure *per se* would undoubtedly increase the con-

⁴ *Nature*, Vol. 51, p. 225.

⁵ As a speculation, not essential to the argument, it is allowable to surmise that there may be a considerable proportion of iron, for three reasons: (1) on account of its large proportion in the earth's crust compared with other metals, (2) on account of the existence of terrestrial magnetism (this is perhaps the weakest argument), (3) by analogy with meteorites.

ductivity, and, if the greater density of the interior were due to difference of pressure, this increase would be very great. This statement will need small emphasis. In the absence of chemical or intramolecular change, it is obvious that more compact packing of the molecules will facilitate the passage of heat. Plain evidence of this action is shown in the increase of electric conductivities of metals with fall of temperature. With the comparatively small contraction of volume, we find an increase of electric conductivity very much greater than the contraction in volume. The variations in this true electric conductivity⁶ are always accompanied by corresponding, though not necessarily proportionate, variations in heat conductivity. Indeed, I do not think any competent chemist or physicist will be inclined to dispute the conclusion that pressure and consequent decrease in volume will probably have the effect described.

The effect of increase in temperature is doubtful. The conductivity of metals decreases with rise of temperature, but the change is small compared with their much greater intrinsic conductivity. The evidence with regard to the ordinary rocks is not conclusive. Lord Kelvin⁷ thought that their conductivity would decrease with rise of temperature. Professor Perry⁸ is inclined to form the opposite conclusion. In all probability the problem will merge itself into the larger one of the state of matter at abnormal temperatures and pressures. Such a difference between competent specialists is a cogent illustration of the uncertainty of the data on which the method is based.

These three sources of difference, so far as present knowledge will carry us, would affect the conductivity of the interior of the earth in the following way:

<i>Cause</i>	<i>Effect (on conductivity)</i>
Change in chemical composition with a greater metallic content.	Great increase.
Increased pressure.	Increase (or with great compression considerable increase).
Increased temperature.	If metallic decrease, but small in pro- portion to intrinsic conductivity. If non-metallic, doubtful.

The balance of this hypothetical evidence, therefore, clearly inclines to an increase of conductivity of an unknown amount. To hypothetical evidence we can add the positive evidence that, accord-

⁶ This argument, needless to say, refers to true electric, not to electrolytic, conductivity.

⁷ *Nature*, Vol. 51, p. 439.

⁸ *Ib.*, p. 583.

ing to Professor Schuster, the electric conductivity must be considerably greater inside the earth.⁹ Thus the conclusion seems highly probable that the heat conductivity of the interior of the earth is considerably greater than that of the crust.

It will throw an interesting light on the present line of reasoning to note the effect of this probable conclusion on Lord Kelvin's estimate of geologic time. Professor Perry¹⁰ has shown that the estimate would thereby be multiplied by a factor "two to six times the ratio of this internal conductivity to the conductivity of the skin." This interesting result is a striking instance of the manner in which, by the use of complicated mathematical formulæ, errors in the data produce a more than proportionate error in the result.

3. The problem of the solidity or fluidity of the earth's interior is generally supposed to have been solved by observations on the behavior of the earth in its reactions to tidal forces. On this subject Lord Kelvin¹¹ expresses the conclusion that "On the whole we may fairly conclude that, whilst there is some evidence of a total yielding of the earth's mass, that yielding is certainly small, and the effective rigidity is at least as great as that of steel." Elsewhere he says that this rigidity is certainly more than "a continuous globe of glass of the same diameter and probably than one of steel."¹² Sir George Darwin¹³ is of the same opinion and thinks it probable that "the earth is solid at least far down to its center." Against this conclusion can be brought the simple observed fact of the extrusion of lava.

In the face of these observations, it will at first sight appear absurd to maintain the possibility of convective action in a body probably as rigid as steel. But, for the purposes of geologic time, these facts prove very little. It is only necessary to inquire exactly what we mean by liquid convection and how much of this will render a conductivity formula useless, to see that these observations are by no means as conclusive as they seem.

When we have proved that the earth is effectively rigid in its reaction to tidal forces and in its behavior with regard to the transmission of earthquake shocks, it may yet be effectively fluid or viscous with regard to long-continued forces acting for vast eras of time. It must be borne in mind that, in speculating on the condition of matter in the interior of the earth, we are treating of conditions which lie beyond the range of present-day science. Of these condi-

⁹ *Phil. Trans.*, Series A, 1889, p. 512.

¹⁰ *Ib.*, p. 225.

¹¹ "Mathematical and Physical Papers," Vol. 3, p. 329.

¹² *Ib.*, p. 237.

¹³ "The Tides," pp. 232-237.

tions we know little, but we can infer with a fair degree of certainty the existence of a very high temperature and of a colossal gravitational pressure. It is evident that it is an inadmissible extrapolation to dogmatize with regard to the possible reactions of matter under conditions so far removed from those of which we have experience.

Interesting and valuable as these researches on the tides may be, they in no way negative the idea that there may be slow convective action throughout the whole interior mass of the earth. Let us imagine such an action as slow as a foot a year. This, in the course of 20 million years, would carry a mass of material from the center of the earth to the circumference. Indeed, convection to the extent of a foot per century would probably entirely vitiate such a calculation as the one we are now considering. Any such action, even as slow as this, would connote a continual circulation of the mass, and, for the purposes of geologic time, would be equivalent to a great increase of conductivity at depths where the heat begins to approach that of the interior.

To any suggestion that this argument is hypothetical, I would at once admit that, were I attempting to found complex mathematical calculations on these suppositions, such speculations would be inadmissible; but the suggestions are no more speculative than the inference that, because the earth is rigid in its reactions to tidal forces, therefore no such reactions exist. The indefinite grading of the properties of solids and liquids here suggested is indicated by more than one branch of chemical fact and theory.

The fallacy of ignoring the possibility of liquid convection in the interior of the earth is a cogent example of the manner in which factors, commonly negligible, become important when we attack a problem of an entirely different order. Because, in studying the cooling of a cannon-ball, we can ignore any minute liquid convection that may exist, it is assumed that this may still be ignored when we study the cooling earth. But the conditions are entirely different. The small intrinsic conductivity of rock and the enormous mass of the earth making cooling so slow, has reduced the conductivity effect to an infinitesimal. But, when that is done, we must also take account of other factors which, because they also are infinitesimals, may commonly be ignored. In studying the cooling cannon-ball we can also ignore the effect of gravitation; but, in the cooling sun, gravitational friction will supply heat for many millions of years.

The former paragraphs refer only to variation of conductivity and convective action in space. There still remains the question of such variation in time. Even if we admit the improbability of such convective actions now (and there is no reason for such an admission) that assumption by no means precludes their possibility in the

remote past. If this is so, our present temperature gradient would have reference, not to the consistentior status, but to the time when such convective actions ceased, which, for all we know, might have been many millions of years after the original cooling of the earth's crust.

The specific heat of the earth is another problematic factor, but one which is probably of trivial importance.

5. The sources of errors in connection with the determination of temperature gradients are considerable and raise some theoretical questions concerning the conditions under which the taking of an average is permissible. Without attempting to enter very thoroughly into these, it will be generally admitted that this process is inadmissible when the variations are large and when their causes are known. In Lord Kelvin's original papers and, so far as I have been able to ascertain, in any subsequent papers, there is no serious attempt to deal with this problem. There is in one passage a suggestion that some particular variations may be due to the residual heat of an eruption,¹⁴ but, as we shall see later, this, if true, would invalidate the calculation of an average. We now know that at any rate a part of these variations is due to differences in the radioactive content of the rocks, but this question I propose to defer for the present.

Apart from the newly discovered effect of radioactivity, there are at least two known possible reasons for the enormous variations in gradients (from $\frac{1}{15}^{\circ}$ F. per foot to $\frac{1}{100}^{\circ}$ F. per foot) : differences in conductivities of rocks and other means by which heat may escape, and the residual heat due to the eruptions of former times. It is evident that, while it would be allowable to take an average of variations due to the first cause, it would be entirely inadmissible with regard to the second, as all such variations of temperature gradient would be positive. So far as the variations were due to that cause, the correct data for a mathematical calculation would be not the average, but the lowest gradient that could be found.¹⁵

The magnitude of variations due to volcanic action past and present has never been estimated, nor—now the problem is complicated by radioactivity—does there appear to be much chance of our so doing; but the uncertainty suggests a number of speculative questions to which some answer must be obtained before it would be possible to admit the validity of the usual deductions from temperature gradients. That the necessary data are unproved may be

¹⁴ "Mathematical and Physical Papers," Vol. 2, pp. 176-177.

¹⁵ Recent borings, made with due regard to some possible sources of error show a tem. grad. of 1 in 77. This correction alone would more than double Lord Kelvin's estimate (Chamberlain and Salisbury, "Geology," Vol. I., p. 543. N. B. $(77/55)^2 = 2.4$ nearly).

shown by the following queries, which raise a few of the unsolved geologic problems that have some bearing on this question: Is there a single locality on the surface of the earth where, if we go deep enough, we should find no traces of igneous rocks formed since the consistentior status?¹⁶ What is the range in place and time, of the effect on the temperature gradient of the existence of a group of volcanoes? (Observations from southern Italy might throw light on this problem.) In what ways and to what extent may temperature gradients be modified by igneous action that does not reach the surface? In what way, if at all, will continuous erosion and sedimentation modify any conclusions that may be drawn? The effects of this extend many miles deep in the earth's crust—as a rule, deeper than our observations of temperature gradients. As the possibility of slow interior convection vitiates the assumption of a mathematically solid earth, so the fact of such convective action in the crust may vitiate the assumption that the gradients that have been observed may be averaged.

Indeed, in this case, immediately we apply a single mathematical formula to such a vast expanse of time, we see that some of our original assumptions must be reconsidered. Let us imagine ourselves on a time machine which would cover a hundred thousand years in what to our senses was a day, and we shall see that our fixed and steady earth, cooling as if it were a rigid globe of glass or steel, is a figment of the imagination. In the place of this still fixed solid earth we should see a globe, the center of plutonic energies. Its surface would rock beneath our feet like the billows of the sea, literally mountains high, and as it rocked, the solid land would pour into the ocean in one mighty flood. There would be vast eruptions from innumerable centers forming lava covering great areas which would once more be plunged beneath the surface of the ocean. Is it possible for all these changes to take place and yet leave the heated interior mass so unaffected that we can calculate how long it has taken to cool on the assumption that it was fixed and steady? Where is the original rock that first cooled in the remote past? Where are we to look for our data if we wish to make our calculation?

To sum up our analysis of this particular method, so far as possible, apart from recent discoveries in radioactivity, it will be seen that the premises are subject to a large number of uncertainties. Assumptions have been made on matters about which we have very imperfect knowledge, and our present inquiry has disclosed the existence of a large number of variable and doubtful factors which

¹⁶ Recent investigations have shown that the great granitoid series, found in the Archean complex, are not the original crust of the earth, but are mainly intrusive in the surrounding metamorphic schists.

make this method entirely inadmissible for the calculation of a maximum limit to geologic time. Assuming the non-existence of any other source of heat, the utmost maximum that could be obtained from the known facts would be referred not to the temperature gradient, but the plain fact that volcanic action is still going on, and that consequently a considerable fraction of the original heat is still existent in the earth's interior. On this basis, if we accept Lord Kelvin's estimate¹⁷ that such original heat is equal to twenty thousand million times the amount dissipated each year, our only calculable maximum would be something less than this enormous lapse of time. We must subtract something from this maximum to allow for more rapid radiation in past eras, and for the fact of observation that the interior of the earth is still hot. How much should be deducted on these counts is an academic question that I shall not attempt to solve. Whether this is a probable or even a possible maximum is also a question that does not arise at this point. But, apart altogether from the discovery of radioactivity, the temperature argument appears to be so faulty that this is the only allowable maximum result deducible from the study of the secular cooling of the earth.

With the introduction of more recently discovered factors, even this residual validity vanishes. The two known sources of internal heat discovered since the publication of Lord Kelvin's papers are internal tidal friction and radioactivity. Sir George Darwin has shown that the heat equivalent of the loss of rotational energy of the earth, owing to tidal friction, if the earth had originally a day of 5 hours 40 minutes, would be equivalent to a secular loss for 3,560 millions of years.¹⁸ He gives a number of mathematical calculations to show that the greater part of this heat would be formed in the central parts of the earth,¹⁹ and so he comes to the conclusion that this would not appreciably affect the temperature gradient near the surface.²⁰ But this conclusion, like so much other mathematical reasoning, implies an entire absence of interior convective action both now and in the remote geologic past. If we reject this implication, this tidal heat thereby becomes a factor of which we must take account in our study of secular cooling.

Careful investigation of the radioactive content of the igneous and sedimentary rocks has shown that, if similar action occurred in the interior, the heat evolution from this source alone would be many times greater than the amount lost by secular cooling. Into this matter we need not now enter. The discovery of radioactive heating

¹⁷ "Popular Lectures and Addresses," Vol. 2, p. 6.

¹⁸ *Philosophical Transactions*, Series A, 1879, p. 592.

¹⁹ *Ib.*, p. 593.

²⁰ *Ib.*, also pp. 564-566.

is one that no scientist of the past century could be expected to have anticipated. It is, however, a very cogent example of the shallowness of this kind of mathematical speculation that it was so ready to assume that there existed in the universe no sources of energy other than those with which we were acquainted.

Let us now show the theoretical reference of this more practical analysis. In my paper on the "Methods of Applied Mathematics" I summed up the most probable sources of error in the fitting of conceptual mathematics to perceptual science in the following four propositions: (1) the validity of mathematico-physical formulæ is dependent on the range and theoretical certainty of the physical or other principles involved in the premises; (2) it is also dependent on the completeness with which the conditions relevant to any particular problem have been abstracted from reality; (3) both these sources of error are increased with the complexity of the formulæ and with the magnitude of the substituted terms; (4) in cases of extensive extrapolation, such formulæ are liable to give not only minor errors, but results which bear no recognizable relation to concrete reality.

Applying these tests to secular cooling, we find: (1) that, although the full discussion of the theoretical foundation of Fourier's formula is too detailed a mathematical question for the present essay, it contains physical assumptions not of the order of axioms; (2) in its application to geologic time, the abstraction is entirely inadequate, some known, and probably many unknown, material factors have been omitted; (3) the formula is complex, small errors in the data make considerable errors in the result, and the extrapolation enormously exceeds the ordinary limits of space and time; (4) it is therefore highly probable, from these theoretical considerations, that the conclusions bear no recognizable relation to concrete reality.

Let us note the special interest of these conclusions. On the one hand, we have an exposition of the principles of applied mathematics which contains truths so clear that the philosophical critic is inclined to dismiss them with the remark that they are true but obvious. On the other hand, we have a theory which, during the past half century, has filled countless pages in books and scientific articles, molded the course of scientific and geological speculation, effected theories of evolution and of heredity, and entailed enormous labor on the part of mathematicians, scientists and writers (to say nothing of students, readers and mechanical workers). To all this, given the clear understanding of a few simple mathematical principles, it is possible to make the single exhaustive comment: one erasure will suffice. What more cogent example could be desired of

the need of a thorough investigation of the theoretical principles of scientific method? We require less mechanical experiment, less mechanical mathematical calculation, more thought, more philosophy.

With the detailed working out of one example, it is hardly needful to give further examples of the misapplication of mathematical method. The tidal-retardation fallacy is another example more obvious than the last, so clear indeed that to apply these principles in detail is almost pedantic. This example, however, is of special interest in that recent discoveries in radioactivity do not affect the argument. This case it is unnecessary to discuss further, because the mathematicians have already discovered their errors by empirical methods. To elaborate other instances would be superfluous. I can, indeed, best conclude this essay, as I concluded my exposition of main principles, by saying that all acquainted with the multitudinous applications of mathematical reasoning to the complexity of the universe around us can, for themselves, find many other examples.

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DISCUSSION

IF THE BLIND LEAD THE BLIND

A COMMENT ON "LOGICAL FORM" IN PROFESSOR PERRY'S "REALISTIC PROGRAM"¹

WHEN will philosophy tire of that Fata Morgana, mathematical method? It is little short of tragic if realism, bearing, as it does, balm for so many ills to which the philosophic flesh is heir, theory of relations, the theory of 'logical constants' or indefinables, the theory of infinity and continuity, and the theory of classes and principal contribution which modern logic is prepared to make to philosophy concerns the form of exact knowledge," and also: "The should be led astray. But I am apprehensive when told that "The systems, concern everything fundamental in philosophy. No philosopher can ignore these and like theories without playing the part of an amateur. . . . For philosophy deals with the same topics as modern logic, but treats *popularly and confusedly* what modern logic treats with the *painstaking thoroughness and exactness* of the expert"² [italics mine].

Before these statements become incorporated in a realistic platform, I should like to present the three following theses, with the proviso that it is not as a subordinate philosophic discipline, as a

¹ This JOURNAL, Vol. VII., No. 14, July 7, 1910.

² *Loc. cit.*, p. 374.